The cases B. 0^h, B. 21^h, and B. 111°–121° rest upon very few observations, and probably do not give a real mean value at all.

There remains only B. 51°–61°, but I find that a very unfair proportion of these are of *a Lyræ*, which brings me to another line of investigation.

It is said that there is an almost invariable tendency to observe (galvanically) bright stars relatively earlier than faint ones, possibly owing to a larger bisection error which may be systematic. Into this question, however, I have no wish at present to intrude, as it does not come within the scope of the paper. I only allude to it because if true the anomalous nature of the result, B. 51°–61°, might be fairly ascribed to the supposition that for *a Lyræ* in particular the galvanic time was early and hence the relative P.E. too small.

I should have been glad to carry out the comparison between groups of stars of different magnitudes; but there is practically no reliable evidence as to the observing conditions in each case, and as to the relative brightness of the same star under different conditions of daylight, twilight, darkness, and cloud.

One other field remains, not foreign to the subject. In observing slow-moving stars, I have practically no data to go upon, but, so far as my experience goes, stars right up to the pole should not give more widely different results between the two methods than the clock-stars for which the data are at hand.

For stars within a few degrees of the pole, I am inclined to think that the Greenwich method of galvanic observations with the very slight modification necessary to make it applicable to eye-and-ear observations, by substituting a known second from the clock for the galvanic contact simultaneously with which the bisection is made by means of the Right Ascension micrometer, would give practically the same result by either method.

A Note on the Result concerning Diffraction Phenomena recently criticised by Mr. Newall. By F. L. O. Wadsworth.

(*Communicated by the Secretaries.*)

In the November number of the *Monthly Notices* Mr. Newall has a note calling attention to an error in a result recently used by me in developing the theory of the "contrasting" or "delineating power" of telescopes. A criticism of this same result had already been published by Professor Schaeberle in the *Astronomical Journal* (No. 421). The criticisms of both Mr. Newall and Professor Schaeberle are just so far as the inaccuracy of the result alone is concerned; but they are both at fault as to the real error that was committed in obtaining it, and as to its influence on the conclusions of my former papers.

This error was discovered by me several days before I had seen either Mr. Newall's or Professor Schaeberle's article.* A note has been published in the *Astronomical Journal* (No. 424) pointing out the errors in Professor Schaeberle's criticisms ; in the present communication I wish to point out other errors in certain statements by Mr. Newall.

A. At the beginning of his note Mr. Newall refers to certain papers of mine in a way that leads one to infer (1) that they were merely duplicate publications in different journals ; (2) that the conclusions of all these papers were affected by the error in the expression for I^2_{111}, the focal plane illumination due to the light from the sky. The first point is not perhaps an important one, but I should like to say a few words in regard to it. My preliminary note in the *Monthly Notices* (1897 June, pp. 586–9) simply pointed out two facts whose importance, in photographic work at least, had not, apparently, been fully recognised—*i.e.* that the general illumination of the sky, due to the scattering of light by small particles in our atmosphere, is (*a*) of much greater importance in photographic than in visual work, because the intensity of the scattered light varies inversely as the fourth power of the wave length ; (*b*) that this illumination can be considered as equivalent to an infinitely extended uniformly luminous area ; and that it is the *contrast* between the general illumination of the focal field due to such an area and the intensity of the image of any object, rather than the absolute value of the latter quantity, that determines the "visibility" and to a large extent the "definition" (both visual and photographic) of faint objects. In that note I also gave, without proof, some of the conclusions which I had then reached in the development of this general theory of "contrast" and "delineating power," based on these and other results. The next publication referred to was that in the *Astrophysical Journal* (1897 August, pp. 119–35), in which the general preliminary development of this theory was taken up from a purely analytical side. It was intended to make use of the results obtained, in conjunction with those derived from the well-known theory of "resolving power," in a discussion of the best practical instrumental conditions of working in the following special cases :—

(*a*) Nebular and stellar photography.
(*b*) Nebular and stellar spectrography.
(*c*) Coronal photography during and without an eclipse.
(*d*) Photography of solar, lunar, and planetary surfaces.
(*e*) Meteor photography and spectrography.
(*f*) Visual observations of fine markings on lunar and planetary surfaces.
(*g*) Visual observations of faint comets, nebulæ, the Gegenschein, zodiacal light, &c.

* *Astrophysical Journal*, 1897 December, p. 463 ; ibid. 1898 January, p. 77.

(*h*) Measurements of angular diameters and distances with the micrometer, heliometer and interferometer.

In each of these cases the conditions of working differ to a considerable degree; and although much has been written on each, there still seemed to be some points that called for further discussion. (*a*) was discussed in the *Astronomische Nachrichten* (No. 3439) (this paper was afterwards published in *Knowledge*); (*d*) in the *Observatory* (1897 September, October, and November, pp. 303, 365, 404); and (*f*) in the *Astronomical Journal* (No. 414); the remaining cases have not yet been taken up in detail. These papers are not therefore duplicate publications, as intimated by Mr. Newall.*

B. With reference to point (2) Mr. Newall states that my conclusions "concerning the performance of various lenses" are based "on the misinterpretation of a result obtained by Stokes," and that consequently my "conclusions are incorrect," and my "criticism and attempted explanation . . . of practical results obtained with large and small reflectors and refractors falls to the ground." These statements again are incorrect. In the first place, the result to which Mr. Newall refers (the expression for I^2_{111}) *was not obtained by Stokes* (although he gave an analytical proof of the value of the integral involved), but by Lord Rayleigh, who announced it (for the first time, I believe), in his memoir on the Wave Theory in the ninth edition of the *Enc. Brit.*, as follows:—"If we integrate (30)—*i.e.*

$$d\xi \int_{-\infty}^{+\infty} \mathrm{I}^2 d\eta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

with respect to ξ, between the limits $+\infty$ and $-\infty$, we obtain $\pi \mathrm{R}^2$, as has already been remarked. This represents the whole illumination over the focal plane, due to a radiant point whose image is at O,† *or reciprocally the illumination at* O (*the same as*

* The practice of publishing a paper simultaneously in several journals is one that is quite common on this side of the water. Personally, I have followed it only a few times (three altogether, including the recent case in *Knowledge*[1]); but I do not consider it necessary or in good taste to either defend or condemn it. It should be said, however, that the reason for it, when it is done, is not that usually imputed to us by our transatlantic friends, but is simply the desire to reach our co-workers in the same field in this country. There are very few of our institutions which have even *one* complete file of the leading scientific publications in any subject; not a single one, as far as my experience goes, that has complete files of *all*. Many munificent gifts have been made for buildings and large instruments, but the fact has not yet impressed itself upon either the American public or upon our Boards of Trustees, that in order to carry on the work of an institution effectively, a good library and equipment of minor instruments is quite as important and essential, if not more so, than a fine building and (for example) a big telescope.

† O is the centre of the field.

[1] In a number of cases my papers have been *reprinted* in other journals. This is quite a different case, and one in which the editors of the journals are solely responsible and solely concerned.

*at any other point), due to an infinitely extended luminous area."** A corresponding result is reached earlier in the same memoir for the intensity of illumination in the image of a long line.*

In the second place, the conclusions in my different papers are based not upon this one result, but, as already pointed out, upon the indications of the general theory of "contrast" or "delineating" power. In two of the three cases, *i.e.* (*d*) and (*f*), that have so far been considered, the result does not enter *at all*, and Mr. Newall has not therefore any right whatever to refer to these cases as affected by the error in the expression for $I^2{}_{111}$. The only case (yet considered) in which it does enter is (*a*). The conclusions reached in this case would have been erroneous had it not been for the fact that in reaching them I omitted (fortunately or unfortunately) to consider the effect of atmospheric aberration on the *effective intensity of point and small surface areas during prolonged photographic exposures.* This effect had not been lost sight of, but its importance in (*a*) was at first under-estimated. It was intended to take up at once the general investigation of this effect as Part II. of the "General Theory of Telescopic Images" (of which Part I. was published in the *Astrophysical Journal* 1897 August), but the investigation had to be temporarily laid aside.† When I did recently take it up again, I at once discovered that the effect of this factor *in prolonged photographic exposures* would be in general to diminish the *effective photographic intensity* of *faint* point and small surface sources (stars, stellar nebulæ, &c.) in the ratio $\frac{1}{f^2}$, and of line sources in the ratio $\frac{1}{f}$.‡ If the result for $I^2{}_{111}$ had been (as first assumed) independent of f, the contrast would therefore have varied as $\frac{1}{f^4}$ or $\frac{1}{f^3}$, instead of $\frac{1}{f^2}$, as found in practice. This discrepancy led me to reinvestigate Rayleigh's result and discover the error in the value for $I^2{}_{111}$. As it now turns out, the expressions for the *effective photographic contrast* between sky and faint stars, or faint diffused nebulæ, are practically correct as given in my preceding papers; the omission of the factor $\frac{1}{f^2}$ in the denominator (expressing the intensity of illumination due to the sky) just balancing the omission of the factor

* "Wave Theory," *Enc. Brit.* vol. xxiv. §§11 and 12. A reference to these paragraphs was given in both my preliminary note to the *R.A.S.* (*Monthly Notices*, footnote on p. 587) and in my subsequent paper in the *Astrophysical Journal* (footnotes on pp. 130, 133). I can hardly see how these could have so completely escaped Mr. Newall's attention. He has indeed completely overlooked the error in the expression for the intensity in the image of a long luminous *line*, although it is exactly of the same nature as that for an extended luminous *area*.

† See note in *Astrophysical Journal*, 1898 January, p. 78.

‡ For further discussion of this point see Part II. of "General Theory of Telescopic Images; Effect of Atmospheric Aberration," *Astrophysical Journal*, 1898 January, p. 70.

$\frac{1}{f^2}$ in the numerator (expressing the effect due to atmospheric aberration). The main conclusions of the paper on stellar and nebular photography, (*a*), are therefore correct, as well as those on planetary observations, (*d*) and (*f*); *i.e.* at least so far as the theory upon which they are founded (and this has not as yet been questioned as far as I know) is correct. As regards this latter point it seems to me that the agreement between the actual times of exposure required to obtain a given photographic effect with different lenses (Barnard's and Pickering's results), and the computed times as determined by the theory of contrast, is too remarkable and striking to be considered as simply accidental and fortuitous.*

There is one minor conclusion of the paper in the *Astronomische Nachrichten* which is indeed based directly and solely upon the first (erroneous) expression for I^2_{111}, and which is consequently in error. This is the conclusion in regard to the "fogging" of the photographic plate by the sky illumination, and the consequent relation between the times of exposure possible with lenses of different apertures.†

Finally, just a word as to the actual error which was committed in obtaining the value of I^2_{111}, since this has not been pointed out by Mr. Newall. In the case of Lord Rayleigh the error was (apparently) due to the assumption (without proof) of the general principle of reciprocality or reversibility of images, *i.e.* that (as stated in my paper in the *Monthly Notices*)

$$\int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} \mathrm{I}^2 d\xi d\eta \ . \equiv . \int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} \mathrm{I}^2 dx dy \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In my case (I approached the question from a somewhat different point of view) the error was a more purely analytical and hence, in many respects, a less excusable one. Mr. Newall is correct in saying "there is a discontinuity in my solutions." In the paper in the *Astrophysical Journal* I derived three expressions—(13), (16), (17)—for the most general case of an image of a source having any form, extent, or distribution in intensity. I applied the first of these—(13)—to the determination of the correct expressions for the intensity in the images of short lines and small uniformly luminous circular areas.‡ Further, when (13) was applied to the cases of a long line and an infinitely extended source, I also obtained correct expressions [(19) and (31)], but

* See *Ast. Nach.* No. 3439, § 100; *Knowledge*, 1897 August, p. 194.

† A note calling attention to this error has been sent to the *Astronomische Nachrichten*. I may perhaps point out in this connection that this result was not regarded as a particularly important one, and was not therefore included in the summary of conclusions at the end of the paper. I said in regard to it, "the mere ability to lengthen the time of exposure (at least beyond twenty-four hours) by decreasing the size of the objective would not in itself be of great importance."

‡ These results were those used in the discussion of cases (*d*) and (*f*).

I assumed that these expressions (in which the variables of integration were x and y, and the field of integration the radiating source) became identical [see (1) above] with the corresponding ones [(8) and (30)]* used by Rayleigh (in which the variables of integration are ξ, η, and the field of integration the focal plane). This would be the case when the limits of integration are infinite, if (as at first appears) the variables x, y were *symmetrically* involved with the variables ξ, η, in the expression for $I^2{}_1$. An inspection of (14) (of my paper), which expresses the value of r (the variable in I^2) in terms of x, y, ξ, η, shows that this is not the case. The first two variables are each multiplied by a factor of dissymmetry, $\frac{f}{D}$, and in order to obtain two new variables symmetrically involved with ξ, η, we must substitute

$$x = \frac{D}{f} d\xi_1, \qquad y = \frac{D}{f} d\eta_1,$$

which gives us in (19) and (31) respectively,

$$\int_{-\infty}^{+\infty} I^2 dy = \frac{D}{f} \int_{-\infty}^{+\infty} I^2 d\eta_1 = \frac{D}{f} (z_{,,}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (19a)$$

and

$$\int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 dx dy = \frac{D^2}{f^2} \int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 d\xi_1 d\eta_1 = \frac{D^2}{f^2} (z_{,,,}) \quad . \quad . \quad . \quad (31a)$$

and the integrals [$(z_{,,})$ and $(z_{,,,})$] of *these last expressions* become respectively identical with the integrals (8) and (30) of Lord Rayleigh.

Yerkes Observatory: 1897 *December* 24.

The Concave Grating for Stellar Photography. By Charles Lane Poor and S. Alfred Mitchell.

(*Communicated by Dr. C. L. Poor.*)

The concave grating has proved a most powerful instrument for spectroscopic research, but heretofore it has not been successfully applied to stellar spectroscopy. Experiments are now being carried out at the Johns Hopkins University, under the direction of Dr. Charles Lane Poor, with the view of thoroughly testing the various methods of using the concave grating for astronomical purposes. The methods, originally suggested by Professor Rowland, were developed and the formulas derived by Dr. Poor, and the apparatus constructed under his direction; the photographs were made by Mr. S. Alfred Mitchell.

* "Wave Theory," *Enc. Brit.* §§ 11 and 12.